

Financial Constraints and Corporate Disclosure*

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Abstract

This paper exploits two quasi-experiments to examine the causal impact of financial constraints on corporate disclosure. First, we use the 2004 American Jobs Creation Act as an exogenous relaxation of financial constraints. Following the Act, firms with un-repatriated foreign earnings reduce the quantity and quality of disclosure, particularly ex-ante constrained firms. Second, we examine the 1989 junk bond market collapse as a tightening of financial constraints and find that below-investment grade firms increase disclosure. Overall, our results suggest a positive causal impact of financial constraints on disclosure, lending support to theoretical models predicting a managerial response to information-related financing costs.

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1. Introduction

Finance theory predicts that information asymmetry between managers and outside suppliers of capital has the potential to affect the investment and financing policies of the firm. For example, if managers possess superior information about the prospects of the firm, an adverse selection problem can arise that may have a negative impact on the cost and availability of financing (Myers and Majluf, 1984). This view is in stark contrast to the standard neoclassical investment model (e.g., Hayashi, 1982), where corporate investment decisions are rationally determined by growth opportunities and all positive net present value projects are funded. In this paper, we examine the hypothesis that corporate disclosure can alleviate such information-related financing frictions, so that more transparent firms will have greater access to market-based financing.

Theoretical work incorporating corporate disclosure choices into models of financing and investment under information asymmetry make this prediction (Lambert et al., 2007; Verrecchia, 2001). Indeed, such models yield a negative relationship between the cost and availability of capital and the degree of information asymmetry between insiders and outside investors, as well as among outside investors. Greater information asymmetry between insiders and outsiders is likely to matter more if the firm is funded with information-sensitive securities, in the Myers and Majluf (1984) sense. Relatedly, greater information asymmetry among investors could lead to a loss of liquidity (Diamond, 1985; Diamond and Verrecchia, 1991; Easley and O'Hara, 2004), especially if relatively uninformed retail investors are deterred from market participation (Balakrishnan et al., 2013; Kelly and Ljungqvist, 2012). Thus, to mitigate such information-related financing costs, capital-constrained managers may be motivated to increase disclosure and improve transparency. This is the central hypothesis we test in this paper.

While prior empirical research has linked corporate disclosure to lower costs and greater

availability of both equity and debt funding (Bharath et al., 2008; Botosan, 1997; Lang and Lundholm, 2000; Lee and Masulis, 2009), identification of a causal link between disclosure and corporate financing has proved challenging (for a survey, see Leuz and Wysocki, 2008). This is in part due to a classic omitted variables problem, whereby some difficult-to-measure factor—such as a change in investment opportunities—is likely to affect both the cost and availability of capital (i.e., financial constraints) and also influence corporate disclosure choices.

In this paper, we tackle this serious endogeneity problem by measuring the adjustment in corporate disclosure in response to two plausibly exogenous shocks to firm-level financial constraints.¹ Our first source of exogenous variation comes from the passage of the American Jobs Creation Act of 2004 (AJCA), which improved access internal funds “trapped” in foreign subsidiaries (Blouin and Krull, 2009). In particular, following the passage of the AJCA, there was a decrease in the tax-related costs of returning unrepatriated profits located in foreign subsidiaries to a U.S.-based parent firm. This one-off tax holiday spurred a large amount of cash repatriations for firms with eligible foreign earnings (Dharmapala et al., 2011). As we argue below, the passage of the AJCA represents an exogenous *relaxation* of financial constraints, particularly for firms that were capital constrained in their domestic operations (see Faulkender and Petersen, 2012).^{2,3}

¹Our approach in this paper is to perturb one of the benefits to corporate disclosure—namely, a financing-related benefit—while holding constant the costs. Examples of costs associated with disclosure include releasing strategic or technological information to industry rivals (Campbell, 1979) or exposing management to outside scrutiny (Leuz et al., 2008).

²There has been a debate in the academic literature as to whether the AJCA had real effects on investment and hiring, consistent with a relaxation of financial constraints. Dharmapala et al. (2011) find that the majority of repatriated income was paid out to shareholders in the form of dividends or repurchases. Moreover, they find no increase in investment, suggesting that repatriating firms were not capital constrained. In contrast, using a superior identification strategy, Faulkender and Petersen (2012) show that only a small fraction of repatriated income was paid out and this effect was concentrated among the firms that were not capital constrained prior to the passage of the AJCA. Importantly, they also find large increases in investment among the subset of ex ante capital constrained firms. These findings are consistent with the AJCA relaxing financial constraints—naturally, only for the subset of constrained firms—by lowering the cost of accessing internal funds.

³We also examine an exogenous *tightening* of financial constraints: the contraction in below-investment

To implement our empirical tests, we start with the universe of publicly traded U.S. firms with the required financial data from 2002 to 2006. We then separate out a set of 489 “treated” firms with unrepatriated foreign earnings that were eligible for the AJCA tax subsidy. These firms have been identified by the finance and accounting literature as having their financial constraints most greatly affected by the AJCA (e.g., Cohn and Wardlaw, 2013; Faulkender and Petersen, 2012). We then compare the post- versus pre-AJCA adjustment in disclosure behavior of this treatment sample relative to a matched control group of observationally similar firms “unaffected” by the AJCA. Thus, using this difference-in-differences approach, we are able to identify the causal impact on corporate disclosure resulting from a relaxation of financial constraints.

We deliver causal evidence that financial constraints influence corporate disclosure. In our baseline tests, we examine both the quality and quantity of disclosure, respectively, using measures of financial reporting quality (Dechow et al., 1995; Jones, 1991) and voluntary disclosure (managerial earnings guidance, as in Anantharaman and Zhang, 2012; Balakrishnan et al., 2013). In the wake of the AJCA, we observe an adjustment in disclosure along both the intensive and extensive margins. More precisely, the estimates from the AJCA experiment imply that a relaxation of financial constraints leads managers to reduce financial reporting quality and provide less voluntary disclosure. This evidence is consistent with theoretical models predicting that managers will limit transparency following a reduction in financing costs, here the relative cost of internal funds (Verrecchia, 2001).

Next, we examine how the effect of the AJCA on corporate disclosure depends on ex ante measures of financial constraints. Theory suggests that the tax-induced relaxation of financial constraints should only affect corporate disclosure for firms that are ex ante financially constrained. For such firms, the cost of funding domestic investment opportunities

grade credit supply after the 1989 collapse of U.S. junk bond market (see Lemmon and Roberts, 2010). See discussion below and the analysis Section 3.5.

may be prohibitive. This is likely because these firms lack internal funds or the information-related costs of accessing external capital markets may be too high. We classify firms in our treatment sample as financially unconstrained or constrained, and test whether our estimated treatment effect is stronger for firms that are more financially constrained. We use several proxies for financial constraints: small firms, non-dividend payers, and the availability of internal funds. We find that the impact of the AJCA on corporate disclosure is in fact larger for firms that are more financially constrained.

To further corroborate our identification strategy, we examine several alternative definitions of the treatment and control groups. Following Foley et al. (2007), we consider an alternative treatment assignment based on an estimate of the actual tax savings associated with repatriating foreign earnings. Also, in a separate test, we redefine our control group to be the set of firms with foreign operations, but lacking foreign profits to repatriate. In each of these tests, we find that our estimated treatment effect remains positive and statistically significant.

We conduct two additional tests to examine the validity of our quasi-experiment. First, we compare pre-AJCA trends in the outcomes (changes in disclosure) of our treatment and matched control groups. We examine trends going back several years prior to the passage of the AJCA and we find no evidence of differential trends in the disclosure behavior of firms between the two groups. Second, we examine the plausibility of our identifying assumption. This assumption states that firms with AJCA-qualified unpatriated foreign profits experienced a relaxation of financial constraints as a direct consequence of the Act, and for no other reasons. This assumption is unlikely to hold in other periods, so, if our identification strategy is valid we would not expect to observe a similar effect of unrepatriated foreign profits on corporate disclosure during non-AJCA periods. We conduct a placebo analysis that implements our treatment assignment scheme and difference-in-differences estimator using alternative event dates. For these placebo events, we find no evidence of different behavior

for our treatment and matched controls firms.

We wrap up our empirical analysis by studying a complementary quasi-experiment that allows us to investigate an exogenous tightening of financial constraints. The event we consider is the 1989 collapse of the junk bond market and subsequent regulatory reform (Almeida et al., 2011, 2013; Lemmon and Roberts, 2010). This event was largely unanticipated and caused a sharp contraction of the supply of junk bonds, a critical source of funding for below-investment grade firms (e.g., Chernenko and Sunderam, 2012). We apply our difference-in-differences methodology to this setting and find that the tightening of financial constraints led firms with a reliance on junk bonds to improve the quality of their financial reporting. We interpret this finding as direct evidence of a set of capital constrained firms improving disclosure practices in order to mitigate information frictions affecting access to external finance and investment. Thus, this alternative and negative shock to financial constraints corroborates our central hypothesis that there exists a positive causal relationship between financing constraints and corporate disclosure.

Our paper contributes to the literature that examines the causes and consequences of corporate disclosure policies. This research mostly focuses on the level of voluntary disclosure in the cross-section, for example, when firms tap capital markets (e.g., Lang and Lundholm, 2000) or change their payout policy (Brockman et al., 2008). Within this literature there are several recent papers that investigate the determinants of corporate disclosure using quasi-experimental research designs. Notably, Balakrishnan et al. (2013) find that managers respond to an exogenous reduction in the supply of public information by voluntarily disclosing more information. They pin down an exogenous loss of public information using reductions in analyst coverage associated with brokerage house closures. Leuz and Schrand (2009) consider the “transparency crisis” that occurred in the aftermath of the Enron debacle of the Fall of 2001 as a natural experiment and examine the firms’ disclosure responses. They find that in the wake of this accounting scandal, firms increased the quantity of disclosure

and capital markets responded by reducing these firms' cost of capital. Our paper advances this literature by identifying financial constraints as a determinant of managers' corporate disclosure choices. To this end, we employ two distinct quasi-experiments that have not been considered in this literature.

Our paper is also related to a literature that examines the interaction of financial reporting quality and (under-)investment. Recently, Balakrishnan et al. (2014) use changes in the value of firms' real estate assets as exogenous shocks to collateral values (see Chaney et al., 2012), and find that the investment and financing decisions of firms with better reporting practices are less sensitive to changes in financing capacity. They argue that better quality disclosure mitigates information asymmetry and related financing costs that may lead to suboptimal levels of investment (see also Biddle et al., 2009). In this paper, we document how financing constraints may influence the choice of disclosure by management. In particular, we find that managers respond to looser (tighter) financial constraints by decreasing (increasing) both the level and quality of corporate disclosure.

The rest of the paper is structured as follows. Section 2 details the empirical methodology. Section 3 describes our baseline results, cross-sectional tests, and robustness for the AJCA. This section also examines the 1989 collapse of the junk bond market as an exogenous tightening of financial constraints. Section 4 concludes.

2. Empirical methodology

In this section, we provide the details of our identification strategy and difference-in-differences estimator. We proceed by describing the design and consequences of the American Jobs Creation Act of 2004 (AJCA), identification strategy, sample selection, and the definitions of key variables used in the analysis.

2.1. The American Jobs Creation Act of 2004

When the profits of U.S. firms' foreign subsidiaries are repatriated to their U.S. parent they are subject to U.S. corporate taxation. At the time of repatriation, if the U.S. corporate tax rate exceeds the foreign rate then the firm owes U.S. taxes on these repatriations at a rate equal to the difference between the two rates. In such circumstances, firms face a tax disincentive to repatriate earnings to the U.S. parent, since deferral of repatriation results in a lower present value of tax payments and a higher after-corporate tax return on investment (Foley et al., 2007).

The aim of the AJCA was to alter these tax-related (dis-)incentives, stimulate repatriations, and spur U.S. investment and job growth.⁴ The policy implemented a reduction in the tax burden associated with repatriating foreign profits back to the U.S. parent firms. In particular, under the AJCA only 15% of “qualified” repatriated foreign profits were considered taxable income. This temporarily allowed firms to return foreign earnings to the U.S. under a reduced marginal corporate tax rate of 5.25%, well below the prevailing 35% U.S. corporate tax rate.

Only qualified repatriated foreign profits of U.S. firms were eligible to receive the tax subsidy. To qualify funds, firms were required to disclose a domestic reinvestment plan. This plan provided details of how the repatriated funds would be allocated towards spending in the U.S. on: “worker hiring and training, infrastructure, research and development, capital investments or the financial stabilization of the corporation for the purposes of job retention or creation” (American Jobs Creation Act of 2004, SEC. 422.(4): Requirement to Invest in the United States). Such activities were explicitly targeted in order to stimulate economic growth in the U.S. and avoid repatriations being distributed to managers and shareholders, in the form of executive compensation or dividend payments and share repurchases.

⁴Faulkender and Petersen (2012) and Blouin and Krull (2009) provide additional details on the design and implementation of the AJCA.

Three additional restrictions determined the form and the maximum allowable size of repatriations under the AJCA (see American Jobs Creation Act of 2004, SEC. 965.(b): Limitations). First, repatriated earnings had to take the form of a cash dividend. Second, the maximum repatriation was limited to the prior level of reported foreign earnings, in particular, the amount of foreign earnings that were “permanently reinvested outside the United States” as reported on the most recent SEC filing on or before June 30, 2003.⁵ Third, only extraordinary dividends qualified for the preferential tax treatment. Firms were required to calculate a base level of repatriations—based on the five tax years prior to June 30, 2003—and only repatriated amounts above this base level were considered eligible for the tax subsidy.

According to IRS data, a total of \$312bn of foreign profits qualifying for a reduced tax rate of 5.25% were paid to U.S.-based parent corporations as dividends, yielding total tax deductions of \$265bn that were claimed from 2004 until 2006 (Browning, 2008). Faulkender and Petersen (2012) are able to attribute 95% of these repatriations to the subset of U.S. publicly-traded firms using data hand collected from SEC 10-K filings. Moreover, these authors present compelling evidence that these repatriations were used to fund new domestic hiring and investment, particularly for the subset of firms that were ex ante capital constrained, consistent with the stated objectives of the Act.⁶ Thus, the evidence is clear that publicly-traded U.S. parent firms did see a large inflow of funds from foreign subsidiaries as a direct consequence of the AJCA tax holiday and these funds relaxed financing constraints, at least for a subset of firms. Accordingly, our empirical design makes use of the AJCA as a temporary tax-induced relaxation of financial constraints to examine the effect of such constraints on corporate disclosure.

⁵If permanently invested foreign earnings were not explicitly reported but the additional tax liability attributable to such earnings was, then the maximum repatriation was equal to this tax liability divided by 0.35. If nothing was reported (e.g., in the case of privately-held firms), then the maximum repatriation permitted was \$500M.

⁶In Section 3.2, we investigate the role of ex ante capital constraints.

2.2. Identification

The purpose of this paper is to empirically investigate how financial constraints affect corporate disclosure. The most straightforward way to conduct such an investigation is to regress a measure of disclosure on cash flow, analogously to a traditional investment-cash flow regression (e.g., Fazzari et al., 1988). However, the estimates from such regressions are difficult to interpret due to omitted variables bias and measurement error (e.g., Almeida et al., 2010; Erickson and Whited, 2000). In particular, an estimated relationship between cash flow and disclosure might reflect the desired impact of relaxed financial constraints on disclosure or, alternatively, an omitted variables problem whereby changes in investment opportunities jointly affect cash flows and disclosure behavior.

The textbook solution to this problem is to instrument cash flow using a source of variation that is uncorrelated with changes in investment opportunities (e.g., Lamont, 1997; Rauh, 2006). We follow this instrumental variables approach and use the AJCA as an exogenous increase in the availability of internal funds (e.g., Blouin and Krull, 2009; Dharmapala et al., 2011; Faulkender and Petersen, 2012). These prior studies have established that the passage of the law did not reflect changes in the domestic investment opportunities of U.S. parent firms. Therefore, in the absence of such confounding effects, the estimated adjustment in behavior in the wake of the AJCA reflects a causal impact of relaxed of financial constraints on corporate disclosure.

To estimate the effect of the AJCA on corporate disclosure practices, we use a difference-in-differences (DID) matching estimation approach. In such a DID design, the sample is first divided into a treated and a non-treated group. For each treated firm, a set of control firms are selected from the non-treated group. Once a suitable control group has been selected, the average change in the dependent variable of the treated group is compared to equivalent change for the matched control group. Thus, we estimate the average effect of the treatment—a tax-induced relaxation of financial constraints due to the AJCA—on the

corporate disclosure practices of the treated group (ATT).

Incorporating a control group into our analysis is essential for two reasons. First, it accounts for potential time effects that impact all firms whether they are treated or not. For example, changes in accounting regulations might influence corporate disclosure for all firms in a way that coincides with the passage of the AJCA (e.g., the Sarbanes-Oxley Act in 2002 as in Cohen et al., 2008). Second, having a matched control group addresses the concern that ex ante differences between treatment and control samples could affect the estimated impact of the AJCA. In our context, this could be due to the fact that larger firms tend to have foreign operations (and are thus more likely to be a treated firm), but that these larger firms are less capital constrained on average.⁷

To implement such a DID design, we will now define treatment and non-treated group assignment, a suitable event window over which we will compare levels of our dependent variables (i.e., corporate disclosure to be defined in Section 2.3), and then define the procedure used to match treated firms to a set of control firms.

The treatment group should consist of firms which had qualified foreign income available for repatriation under the AJCA and with strong incentives to do so. As discussed in Section 2.1, these foreign earnings should be in the form of cash and more likely to be located in countries where the corporate tax rate is low relative to the prevailing 35% U.S. corporate tax rate. Conversely, the non-treated group should consist of firms with a small amount of foreign earnings in the form of cash available for repatriation located in low tax countries. The non-treated group may include firms that have no foreign subsidiaries or unprofitable foreign subsidiaries. Alternatively, they may have foreign earnings but in countries where the tax rate is higher than in the U.S. and so that there would be little incentive to repatriate these

⁷Using matching methods can improve the estimation of treatment effects, as compared to a linear regression model with controls, especially when control variables have poor distributional overlap (Heckman et al., 1998). Moreover, including control variables in a linear framework might not control for unobservable heterogeneity, especially if there exist nonlinearities in the data (Roberts and Whited, 2012). For these reasons, we combine our quasi-experiment with a counterfactual matching approach.

funds under the AJCA. In principle, such non-treated firms should have a low probability of repatriating foreign income under the AJCA. Along these lines, Faulkender and Petersen (2012) demonstrate that the firm-level probability of repatriating foreign earnings under the AJCA is determined by the supply of unpatriated foreign earnings. In particular, the decision to repatriate depends on the level of permanently invested foreign earnings and cumulative foreign earnings over the three years prior to the passage of the AJCA (both existence and size), as well as an estimate of the relative tax advantage of repatriating earnings under the AJCA as compared to the prior tax regime.⁸

In accordance with these empirical findings, we assign treatment status using the level of foreign profits in the years immediately prior to the AJCA (see also Cohn and Wardlaw, 2013). For each firm in the sample, we follow Faulkender and Petersen (2012) and cumulate foreign profits at the firm level over the three years prior to the passage of the AJCA, i.e., from 2001 until 2003. Foreign profits are defined as Compustat variable *pifo*. Using this three year period has the advantage of providing a sufficiently long window in order to reliably measure recent foreign earnings while avoiding profits from several years prior that have already been returned to the U.S. parent. We assign the firm to the treatment group if these cumulative profits are positive. Firms with nonpositive cumulative profits are assigned to the non-treated group along with firms without any foreign operations (unreported foreign profits). Using cumulative foreign profits captures qualified cash available to be repatriated from foreign subsidiaries to the U.S. parent.⁹

To isolate tax-induced changes in financial constraints, we focus on a two year event window either side of the AJCA of 2004. In contrast to event studies that use daily stock market data, we rely on voluntary disclosure and annual accounting data that require a

⁸Foley et al. (2007) demonstrate that this measure of the relative tax advantage is a key determinant of the stock of permanently invested foreign earnings due to tax-related incentives.

⁹In Section 3.3.1, we classify treated firms on the basis of the relative tax advantage measure of Foley et al. (2007). In a separate test, we also consider an alternative non-treated sample restricted to consist of firms with foreign operations, but lacking foreign earnings to repatriate. Both tests yield similar results.

longer event window. We use financial statement and disclosure data from the two fiscal years ending prior to the law change as the “before AJCA” period (i.e., 2002 and 2003) and the two years following as the “after AJCA” period (i.e., 2005 and 2006). For example, consider a treated firm with a December fiscal year-end. In such a case, the pre-AJCA observations will include years ending on December 31, 2002 and 2003 and the post-merger years are set to December 31, 2005 and 2006. This yields two non-overlapping periods for all the firms included in our sample, one pre- and one post-AJCA.

We match each treated firm to a set of control firms selected from the non-treated sample. Matching is conducted on the basis of observable characteristics that are measured at the firm level in the year prior to the AJCA (see Section 2.3 for precise definitions of these matching variables). We match on firm size (*SIZE*), market divided by book value of assets (*MTB*), operating performance (*ROA*), asset tangibility (*TANG*), and the prior level of corporate disclosure (e.g., *FRQ*). We match on these characteristics for three reasons. First, treated firms in our sample have foreign operations and tend to be larger than non-treated firms on average (see Table 1). Second, treated firms necessarily have profitable foreign operations and thus tend to be more profitable overall, relative to non-treated firms. Third, size, performance, as well as the remaining matching covariates are known to correlate with measures of corporate disclosure (e.g., Kothari et al., 2005).¹⁰

We implement a matching procedure to control for these distributional differences, which could jointly affect selection into treatment group and the post-AJCA disclosure outcomes. We use a nearest-neighbor propensity score matching procedure, originally proposed by Rosenbaum and Rubin (1983) and recently used in quasi-experimental empirical studies of corporate disclosure (e.g., Balakrishnan et al., 2013; Irani and Oesch, 2013). This matching

¹⁰Our results are quantitatively similar when match on other variables, including stock market performance prior to the passage of the AJCA or measures of financial constraints (e.g., matching additionally on firm age and higher-order terms, along the lines of Hadlock and Pierce, 2010). In Section 3.2 we examine the role of ex ante measures of financial constraints in detail.

scheme first estimates a logit regression of a dummy variable for whether a given firm is assigned to the treatment group (dummy set equal to one) or non-treated group (dummy set equal to zero) on our matching covariates.¹¹ To estimate this regression we use a sample consisting of 489 treatment and 1,149 non-treated firms from the pre-AJCA period, which is the set of treated and non-treated firms with all matching covariates available both before and after the passage of the AJCA. The estimated coefficients from the logit regression are used to impute the probability of treatment (propensity score) for each firm in the sample. These propensity scores are then used to implement a nearest-neighbor match with replacement using a standard tolerance (0.005 caliper) and allowing for up to three unique matches per treated firm. Multiple matches per treated firm are used to increase the accuracy of our estimated average treatment effects. This is feasible in our setup, as the number of candidate control firms exceeds the number of treated firms.

Using the matching procedure described above, we construct the following DID estimator. For each firm i in the treatment sample, the partial effect of the AJCA on disclosure, y , is calculated as the difference between two components,

$$DID^i = (y_{T,2}^i - y_{T,1}^i) - (y_{C,2}^i - y_{C,1}^i), \quad (1)$$

where the first component is the difference in disclosure of i in the treatment sample (T) moving from the pre-AJCA to the post-AJCA period. The second component is the average difference in disclosure of the control firms that are matched to firm i . To assess the average effect for all firms in the treatment sample (ATT), we take the average of all individual DIDs.

¹¹Our findings are very similar when we utilize a probit regression to estimate propensity scores.

2.3. Sample selection and variable construction

In this section, we describe the sample selection procedure, construction of the matching and corporate disclosure variables, and also present summary statistics for our sample.

We construct our sample by collecting firm-year observations for publicly traded U.S. corporations from Standard & Poor’s Compustat. We assign two fiscal years to the windows before and after the year the AJCA came into effect (i.e., 2002, 2003, 2005 and 2006). We assign the last completed fiscal year ending in the calendar year 2003 to the pre-AJCA window and the first complete fiscal year ending in 2005 to the post-AJCA window. We collect 20,656 firm-year observations this way.

We first apply some basic filters to data. As is common in the financial constraints literature (e.g., Almeida et al., 2004), from the raw data we exclude firm-year observations with total book assets less than \$10 million, showing asset growth greater than 100%, and plant and equipment exceeding total assets. We screen out firms that are financial institutions or regulated utilities (SIC 6000-6999 or greater than 4900-4999, respectively).

We construct matching variables using both balance sheet and securities price data from the merged CRSP/Compustat database. When choosing matching variables, we identify key control variables used in prior empirical research that also examines corporate disclosure as a dependent variable (e.g., Armstrong et al., 2012; Bergstresser and Philippon, 2006; Yu, 2008). Accordingly, we include firm size (*SIZE*) defined as the natural logarithm of the book value of total assets (Compustat item *at*). We include the ratio of market to book value of assets (*MTB*), where the market value of assets is calculated as the market value of common equity (common shares outstanding multiplied by stock price, item *cs ho* \times item *prcc_f*) plus preferred stock (item *pstkl*) and book debt (item *dlc* + item *dltt*), minus the book value of deferred taxes (item *txdb*). Where the book value of deferred taxes or preferred stock is not reported we set the relevant value equal to zero. Operating performance (*ROA*) is calculated as the ratio of income before extraordinary items (item *ib*) to lagged total assets.

Asset tangibility (*TANG*) is net property, plants, and equipment (item *ppent*) divided by total assets. These accounting variables are all winsorized at the 1% and 99% levels to reduce the potential influence of outliers. Constructing these matching variables imposes data constraints that reduce the sample to 9,635 firm-year observations.

In the baseline set of tests, we examine the impact of financial constraints on the quality of corporate disclosure. To this end, we employ an extensively-used accrual-based measure of financial reporting quality, *FRQ*, as our main dependent variable (Dechow et al., 1995; Jones, 1991).¹² Accounting adjustments are used to divide earnings into cash flows and accruals. Both the magnitude and direction of these accruals are subject to management’s judgement and can therefore offer a means to manipulate reported earnings and influence the informativeness of corporate financial reporting. Consequently, discretion by managers in the use of accruals can make it harder for outside stakeholders examining financial statements to extract the true economic performance of the firm (Dechow and Skinner, 2000; Dichev et al., 2013; Healy and Wahlen, 1999).

FRQ is constructed as follows. First, we estimate industry-level cross-sectional models of accruals, which we then use to predict the “normal” level of accruals for each firm in our sample. This type of cross-sectional measure has the advantage of helping us overcome the data restrictions and survivorship bias associated with time-series models (Subramanyam, 1996). In addition, given that our interest is in the relatively short horizon change in accrual manipulation around the AJCA, a long horizon time-series estimate is inappropriate.

We compute the normal level of accruals using the Jones model (Jones, 1991) in its modified version (Dechow et al., 1995). First, we first estimate the following cross-sectional

¹²In Section 3.4, we also match our sample to data on company issued earnings guidance and examine the impact of financial constraints on the quantity of voluntary disclosure. We defer descriptions of these data until the later section where we discuss these tests.

linear model for each two-digit SIC industry-year combination

$$\frac{TA_{it}}{ASSETS_{i,t-1}} = b_1 \frac{1}{ASSETS_{i,t-1}} + b_2 \frac{\Delta REV_{it}}{ASSETS_{i,t-1}} + b_3 \frac{PPE_{it}}{ASSETS_{i,t-1}} + \epsilon_{it}, \quad (2)$$

where TA_{it} denotes total accruals of firm i in year t , calculated as the difference between net income (Compustat item ni) and cash flow from operations (item $oancf$), ΔREV is the difference in sales revenues (item $sale$), and PPE is property, plants and equipment (item $ppegt$). All variables are scaled by lagged total assets (item at).¹³

Once (2) is estimated, the resulting coefficients are then used to predict the level of normal accruals (NA) on a firm by firm basis, given by

$$\frac{NA_{it}}{ASSETS_{i,t-1}} = \hat{b}_1 \frac{1}{ASSETS_{i,t-1}} + \hat{b}_2 \frac{\Delta REV_{it} - \Delta AR_{it}}{ASSETS_{i,t-1}} + \hat{b}_3 \frac{PPE_{it}}{ASSETS_{i,t-1}}, \quad (3)$$

where ΔAR is defined as the change in receivables (item $rect$). Finally, financial reporting quality, FRQ , is calculated as the absolute difference between total accruals and the imputed firm level of normal accruals:

$$FRQ_{it} = |TA_{it} - NA_{it}|. \quad (4)$$

This measure is also commonly referred to as “abnormal accruals.” Large values of absolute abnormal accruals indicate a relatively large discrepancy between the cash flows and the earnings of a firm. Thus, larger values make it more difficult for outsiders to infer the underlying performance of the firm, which is consistent with less informative corporate financial reporting.

We calculate FRQ for each firm-year observation in our sample. In order to do so, each firm-year observation must have the variables necessary to perform this calculation.

¹³Our results are similar when we employ alternative industry classifications, such as one-digit SIC or the 48 Fama-French industry classification.

This leads to a final sample of 6,632 firm-year observations, consisting of 1,658 unique firms satisfying all data requirements. This reduction in sample size results from missing accounting data, a missing SIC-code, or an observation belonging to an industry-year combination with fewer than 15 observations. As with the other accounting variables, we also limit the potential influence of outliers by winsorizing *FRQ* at the 1% and 99% level.

As described previously, treated status is assigned at the firm level, using cumulative foreign profits (Compustat item *pifo*) over the three years prior to the passage of the AJCA (Cohn and Wardlaw, 2013; Faulkender and Petersen, 2012). Firms with positive profits are assigned to the treated group (509 unique firms) and firms with nonpositive or missing foreign profits are assigned to the non-treated group (1,149 unique firms). Then, applying our matching procedure to the set of non-treated firms, we select our control sample (1,497 matches, consisting of 595 unique firms).

Table 1 presents the summary statistics for *FRQ* and the matching variables for both treated, non-treated, and control samples in the pre-AJCA period. Panel A of Table 1 compares the treated with the non-treated sample, and Panel B with the matched control sample. For the reasons discussed in Section 2.2, differences between treated and non-treated firms occur as a natural consequence of treatment assignment. Treated firms in our sample have foreign operations and thus the entire distribution of *SIZE* for treated firms appears to lie to the right of the distribution of non-treated firms. In addition, since treated firms necessarily have profitable foreign operations, they tend to be more profitable overall. This is evident when comparing the distribution of *ROA* for treated and non-treated firms. Two-tailed difference in means tests confirm these statements. In particular, all variables in Table 1 have different means at the 1% level of statistical significance, with the exception of *MTB*. In contrast, Panel B shows that the matching procedure successfully identifies a suitable control group. In particular, there do not appear to be any differences in the mean values of the covariates we consider between treated and matched control groups.

These summary statistics are consistent with our matching procedure bringing us towards an ideal test in which treated and control samples differ only with regard to their eligibility to repatriate foreign profits under the AJCA.

3. Results

This section presents the key results of this paper. We begin by documenting a deterioration in the average quality of corporate disclosure among treated firms in the wake of the AJCA (Section 3.1). We then demonstrate that the magnitude of firm responses depends on ex ante measures of financing constraints at the firm level (Section 3.2). In Section 3.3, we conduct a robustness analysis in which we consider alternative treatment and control assignments, and test the validity of our quasi-experimental research design. We then show that treated firms' quantity of disclosure also declines following the AJCA (Section 3.4). Finally, in Section 3.5 we consider the 1989 collapse of the junk bond market as an alternative setting in which below-investment grade firms face an exogenous tightening of financial constraints and document how these firms respond in terms of their corporate disclosure choices.

3.1. *The AJCA and corporate disclosure*

We now examine our central hypothesis that the lower cost of internal funds resulting from the AJCA will erode the financing-related benefits associated with better quality disclosure.

Table 2 presents the implementation of our difference-in-differences (DID) matching estimator and key results. Here, we examine the impact of the AJCA on the quality of corporate financial reporting, *FRQ*. Firms in the treatment group are compared with counterfactuals that have been matched on several observable characteristics. Before the passage of the AJCA, we see that levels of financial reporting quality among treatment and control firms are similar in terms of magnitude and statistically indistinguishable (see column 1). This is

unsurprising given that the level of pre-AJCA *FRQ* is a matching variable, but it shows that our matching procedure works well. Column 2 indicates that the disclosure policies of the treated and matched control firms diverge after the AJCA. While the average level of *FRQ* among treated firms increased by 0.037, the corresponding change among control firms was an increase of 0.026 (column 3). Both of these estimated changes are statistically significant at the 1% level. These estimates imply that the quality of corporate disclosure decreased by 0.012 more, on average, for firms that had qualified unrepatriated foreign earnings prior to the passage of the AJCA, relative to otherwise similar firms lacking qualified funds.

The difference-in-differences matching estimate of the average treatment effect on the treated (ATT) implies an increase in abnormal discretionary accruals approximately equal to 12% of one standard deviation. Thus, the effect we document is both statistically and economically significant. A crucial observation regarding Table 2 is that the average financial reporting quality of the control firms deteriorated over the event window. This trend in reporting quality among control firms is not surprising and could, for instance, be a reflection of cyclical patterns in disclosure among large and profitable firms (i.e., among firms with the control group's characteristics).¹⁴ This observation underscores the importance of incorporating a control group and implementing a DID design. Evidently, a naive time-difference estimate for the treatment sample would over-estimate the ATT by three times (0.037 versus 0.012).

The differential change in financial reporting quality that is produced by the matching estimator is a central result of our paper. These estimates imply that the reduction in the tax-related cost of repatriating foreign profits relaxed financing constraints and led to a deterioration in reporting quality following the passage of the AJCA. In particular, given our construction of *FRQ*, this means that treated firms' financial statements incorporate

¹⁴See Dichev et al. (2013) and Cohen and Zarowin (2007) for survey- and archival-based evidence on cyclical patterns in financial reporting quality, respectively.

a greater amount of abnormal accruals, i.e., a larger gap between cash flows and earnings. Our interpretation of this adjustment is that firms react to the lower cost of internal funds by lowering the quality of their financial reporting. This finding is consistent with economic models of capital structure, information asymmetry, and corporate disclosure behavior (e.g., Myers and Majluf, 1984; Verrecchia, 2001). In such models, disclosure mitigates the potentially large financing costs associated with information asymmetry. A lower cost of internal funds reduces the need to access external capital markets and therefore reduces the potential benefits of an informative disclosure policy.

Overall, our baseline results indicate that an exogenous, tax-induced relaxation of financial constraints causes lower quality financial reporting. These findings are consistent our expectation that a lower cost of internal funds reduces the benefits—in terms of alleviating information-related financing frictions—associated with better quality disclosure.

3.2. The impact of ex ante financial constraints

The results of this section show that only ex ante financially constrained firms adjusted their corporate disclosure behavior in response to the AJCA.

Finance and accounting theory predicts that information asymmetry between corporate insiders and outside investors can give rise to moral hazard and adverse selection problems (e.g., Myers and Majluf, 1984), which may negatively impact access to external funding as well as investment. Throughout this paper, we assert that corporate disclosure can alleviate such information-related financing frictions, so that more transparent firms will have greater access to market-based financing (Lambert et al., 2007; Verrecchia, 2001). Existing empirical research lends support to this assertion, with several papers identifying an association between corporate disclosure and lower costs and greater availability of both equity and debt funding (Bharath et al., 2008; Lang and Lundholm, 2000; Lee and Masulis, 2009).

In our context, in the presence of information- and tax-based financing frictions the choice

of funding—accessing capital markets or using internal domestic or foreign cash flow—will depend upon relative cost of each of these sources of capital. In the years immediately prior to the passage of the AJCA, repatriating foreign earnings to the U.S. from a subsidiary located in a low tax jurisdiction had significant corporation tax implications. Under the AJCA, this tax cost was reduced in a meaningful way, as described in Section 2.1. Thus, it had the effect of reducing the cost of accessing internal funds both in absolute terms and relative to other sources of financing. For financially constrained firms that could not easily access domestic internal cash flow or capital markets, the AJCA was more likely to increase investment and hiring funded by repatriated earnings (as documented in Faulkender and Petersen, 2012). Our expectation is that the managers of such firms are also more likely to change their corporate disclosure practices. Since these firms will be more likely to substitute to internal funds, there will likely be a greater reduction in the benefits of disclosure and transparency in terms improving access to outside sources of capital. Conversely, financially unconstrained firms have already optimized their investment and disclosure decisions and therefore, at the margin, the lower cost of internal funds resulting from the law is less likely to induce a behavioral response.

Thus, we will now focus on how the ability to repatriate income under the AJCA changed the corporate disclosure response of both ex ante financially unconstrained and constrained firms. The arguments presented above suggest that the deterioration in financial reporting will be concentrated among the constrained subset of treated firms. To investigate this hypothesis we examine the cross-section of treated firms. We examine whether the estimated positive treatment effect of the AJCA on financial reporting quality is different for constrained versus unconstrained firms based on three classifications. We classify our treatment sample using three ex ante measures of financial constraints. For each of these measures, constrained firms that repatriated funds under the AJCA did increase investment whereas unconstrained firms did not (see Faulkender and Petersen, 2012). This suggests that these

firms were behaving suboptimally with regards to investment and, as we shall investigate here, disclosure policies.

First, we consider firm size. Small firms are more likely to suffer from information asymmetry problems and have difficulty accessing capital markets (e.g., Hadlock and Pierce, 2010), potentially leading to under-investment and suboptimal levels of transparency. For these firms, the tax-induced reduction of the cost of accessing internal funds is likely to reduce the external financing-related benefits of disclosure. Thus, these firms are more likely to adjust to a relaxation of financial constraints with a reduction in reporting quality.

We partition our treatment sample into two sub-samples based on median book assets in the pre-AJCA period (“Small” versus “Large”). We present these results in the columns 1 and 2 of Table 3. As column 1 shows, the estimated partial effect of the AJCA on financial reporting quality for small firms is positive and significant. The estimated ATT is larger in magnitude ($ATT = 0.017$) than the corresponding estimate for the full sample (see Table 2, column 3) and significant at the 5% level. On the other hand, the estimated treatment effect is small in magnitude and indistinguishable from zero in the case of large treated firms. This finding is consistent with small firms—those firms more likely to be financially constrained and face an under-investment problem—reducing the transparency of their reporting after the law change. This suggests that, for small firms, the information-related costs of management reducing disclosure exceed the benefits.

Next, we examine dividend payer status. Firms that pay a dividend are less likely to be financially constrained (see Almeida et al., 2004; Fazzari et al., 1988). Consequently, we expect the reduction in financial reporting quality to be concentrated among non-dividend payers.

We classify a firm as a dividend payer if a dividend was paid in the pre-AJCA period (“Yes”) and a non-payer otherwise (“No”). We present these results in columns 3 and 4. Consistent with ex ante financial constraints playing an important role, we observe that

non-dividend paying treated firms reduced the quality of reporting following the law change relative to their matched control group. We do not observe any such behavior among dividend payers.

Finally, we partition the treatment sample on the basis of domestic internal cash flow. Firms with lacking internal funds are more likely to resort to external capital markets or repatriate foreign earnings in order to fund their domestic investments, which, as discussed above, may have implications for corporate disclosure. We therefore expect firms with low levels of internal cash flow to benefit most from repatriations under the AJCA and reduce transparency in response. In contrast, firms with abundant internal cash flows were likely unconstrained and have already optimized investment and disclosure, thus these firms will be unlikely to react to the law change.

We classify the treatment sample into two groups based on median cash flow (net income) divided by assets in the pre-AJCA period (“High” versus “Low”). In columns 5 and 6 we see that the low cash flow firms have the greatest deterioration in financial reporting quality, as evidenced by the positive and significant estimated ATT. For the high cash flow treated firms, the estimated effect of the AJCA on disclosure is smaller in magnitude and statistically insignificant.

To summarize the results of this section, we find that the response in corporate disclosure to the passage of the AJCA is more pronounced among ex ante financially constrained firms. We observe no adjustment in reporting behavior among financially unconstrained firms. Our interpretation is that management’s investment and disclosure policies are already close to optimal for the unconstrained firms and the shock to the cost of internal funds resulting from the AJCA is less important.

3.3. Robustness and falsification analysis

Given the similarity between treatment and control firms in the baseline tests (see Table 1), the evidence thus far is in line with a causal effect of financing constraints on corporate disclosure. In order to strengthen this interpretation, in this section we conduct a series of robustness tests designed to assess the robustness and internal validity of our empirical design. We show that our results become stronger when we refine the treatment intensity (above median foreign profits) and persist when we consider an alternative tax-based treatment assignment. We also confirm that our baseline results are not driven by differences between U.S.-based firms and firms with foreign operations by restricting our control sample to firms with foreign operations. We show that our results cannot be explained by different trends in corporate disclosure in the run up to the AJCA. Finally, we demonstrate that the AJCA results do not persist in other time periods.

3.3.1. Alternative treatment and control group assignment

Our identification strategy assigns treated status to firms reporting positive cumulative foreign profits in the three years prior to the passage of the AJCA. All remaining firms are assigned to the control group. While this group assignment scheme is motivated by a body of work examining the decision to repatriate under the AJCA, particularly Faulkender and Petersen (2012), it is still a somewhat arbitrary choice that warrants further investigation. Here, we examine the robustness of our results to alternative definitions of treatment and control group assignment.

We first examine the sensitivity of our results to changes in treatment assignment. We consider a modification of our baseline treatment assignment as well as an alternative assignment procedure based on the estimated tax benefit of repatriating under the AJCA.

Repatriations under the AJCA were only permitted in the form of a cash dividend (see Section 2.1), thus firms with more foreign earnings available to repatriate should be in a

better position to take advantage of the tax holiday. This is the legal basis of our baseline treatment assignment scheme, which uses foreign profits as a measure of cash available to repatriate under the AJCA. One might reasonably expect that the benefits of the AJCA—in terms of relaxing financial constraints—would be larger if firms had more unrepatriated foreign profits and vice versa for firms with only small amounts of earnings available to repatriate. Following this logic, we investigate the sensitivity of our baseline estimates to a higher cutoff on cumulative foreign profits. In particular, we assign treated status to firms with cumulative foreign profits above the median. To control for size effects, we now scale foreign profits by book assets each year and take the average over the three years, an adjustment that was unnecessary in the baseline tests.

The results of this test are shown in Table 4. In column 1, we show the baseline estimation results for reference purposes and in column 2 we show the estimation results under the treatment assignment based on the median of cumulative foreign profits from 2001 until 2003. The test now zones in on 252 treated firms with a greater level of unrepatriated foreign profits. As expected, the deterioration in financial reporting quality for treated firms relative to control firms becomes more pronounced than the benchmark case ($ATT = 0.017$ versus 0.012 in the base case and significant at the 5% level).

Next, we consider an alternative treatment assignment procedure based on a firm level estimate of the tax savings under the AJCA. As discussed in detail in Section 2.1, the greater the U.S. corporate tax rate relative to the equivalent foreign rate, the greater the tax incentive of repatriating foreign earnings under the AJCA (Desai et al., 2007). Following Foley et al. (2007), we estimate the tax savings under the AJCA at the firm level using the

formula:¹⁵

$$\text{Tax savings} = \frac{\max \{0, \text{U.S. tax rate} * \text{foreign pretax earnings} - \text{foreign taxes paid}\}}{\text{Assets}} \quad (5)$$

This calculation compares actual foreign taxes paid (Compustat item *txfo*) to the taxes that would have been paid on the foreign income had it been taxed in the U.S. at the prevailing 35% rate (in 2003). Thus, this measures the dollar tax that would have been due upon repatriating all foreign earnings in the year immediately prior to the AJCA. Notice that the measure captures not only the difference in U.S. and foreign tax rates, but also adjusts for foreign earnings at the firm level. Clearly, a firm with a large portion of its earnings located abroad stands to gain a lot even if there is a small difference between U.S. and foreign tax rates.

We calculate the tax savings for each firm in our sample and consider two treatment assignment procedures based on this variable. We then examine corporate disclosure after the AJCA using our benchmark DID estimator. We first assign a firm to the treatment group if it has positive tax savings and to the control group otherwise (499 treated firms). Second, we assign a firm to the treatment group if it has above-median tax savings and to the control group otherwise (234 treated firms). In line with the discussion above, our expectation is that those firms with the greatest tax savings resulting from the law change are going to be most likely to adjust their investment and disclosure policies. Conversely, those firms gaining little are unlikely to re-optimize.

The results of this analysis are shown in columns 3 and 4 of Table 4. We find a deterioration in financial reporting quality for treated firms with positive tax savings associated with repatriating foreign earnings under the AJCA (ATT = 0.012 and significant at the 5% level). Moreover, this effect becomes stronger when we consider firms with above-median

¹⁵Foley et al. (2007) find that firms with large potential tax savings keep a larger portion of their cash in subsidiaries located abroad.

tax savings ($ATT = 0.016$ and remains significant at the 5% level).

To summarize the results so far, the evidence presented in Table 4 supports the hypothesis that treated firms' financial constraints were relaxed following the passage of the AJCA, causing a deterioration in their financial reporting quality. As we increase the treatment intensity—a greater availability of qualified funds or greater tax savings from repatriating under the AJCA—the effects we measure become larger in magnitude.

In the final test of this section, we examine the sensitivity of our estimates to a change in the control sample. In particular, we restrict our non-treated sample to include only firms with foreign operations. The purpose of this test is to alleviate the concern that differences between firms with and without foreign operations drives our results.

Recall that firms are assigned into the non-treated group if they have nonpositive cumulative foreign earnings over the years 2001 until 2003. If firms do not report foreign earnings over this time period then they are also assigned to the non-treated group. Of the 1,149 unique non-treated firms, 145 fall into the latter category. We now remove this group from the analysis and repeat our benchmark DID matching estimation procedure using firms with (unprofitable) foreign operations as the set of candidate control firms. If the deterioration in financial reporting quality among treated firms was a consequence of the AJCA and not due to differential trends between firms with and without foreign operations then we would expect to see similar results to our baseline estimates (see Table 2).

Column 5 of Table 4 presents the estimates using the alternative control sample of firms with foreign operations.¹⁶ Consistent with the benchmark results, the estimated ATT is positive and statistically significant ($ATT = 0.022$, significant at the 5% level), indicating that firms with qualified funds available for repatriation under the AJCA reduced the quality of their financial reporting relative to otherwise similar firms with foreign operations but

¹⁶The treatment sample is unchanged, but the matched control sample now consists of 855 matches consisting of 142 unique firms.

lacking such funds. These findings reassure us that our baseline ATT estimates do not merely reflect differences in trends in disclosure between firms with and without foreign operations over the time period in question.

3.3.2. *Validity of quasi-experiment*

The validity of our DID methodology hinges on the parallel trends assumption. This requires that treated and matched control firms have similar corporate disclosure behavior in the run-up to the AJCA. Differences in outcomes between groups in the post-AJCA period can only be attributed to the treatment when this assumption holds.

The dependent variable of our study is the within-firm change in financial reporting quality. Notice that our matching procedure matches treated and control firms on the basis of pre-AJCA disclosure (see Section 2.2). The high quality of this match is evident from the summary statistics and tests in Table 1, which show that there is no difference in the pre-AJCA level of disclosure between treated and control groups in terms of both magnitude and statistical significance. However, despite similar levels of *FRQ* immediately prior to the law change, it might be plausible that the disclosure behavior of treated and control firms are on different trajectories in the longer term. Evidence of such differential trends in *FRQ* may be uncovered by examining disclosure outcomes over a longer horizon prior to the passage of the AJCA.

Panel A of Table 5 reports an analysis of pre-trends in financial reporting quality in the run up to the AJCA. We fix the treatment and control groups used in the baseline experiment. We then calculate the mean and median two year change in *FRQ* for these two groups, in columns 1 and 2 respectively, going back five years prior to the AJCA. Each row reports a different year prior to the AJCA. In columns 3 and 4, we also report differences in means and medians between the two groups along with corresponding *p*-values.

The estimates in this Panel A of Table 5 provide evidence that the parallel trends assump-

tion holds. We can see that both the treatment and control groups follow similar year-to-year changes in financial reporting quality as far as five years prior to the AJCA. The differences-in-means (and medians) tests confirm this pattern, indicating that the dependent variable of our study is statistically indistinguishable between the treated and control groups.

Our identification strategy is also based on the assumption that firms with AJCA-qualified unpatriated foreign profits experienced a reduction in the cost of internal funds and relaxation of financial constraints in the 2004 to 2006 period. The evidence in Faulkender and Petersen (2012) strongly supports this assertion. Nevertheless, this assumption is unlikely to hold in other periods, so, if our identification strategy is valid we would not expect to observe a similar effect of unrepatriated foreign profits on corporate disclosure during non-AJCA periods.

We conduct two placebo tests that mechanically shift the date of the passage of the AJCA two years backwards (i.e., 2002) and forwards (i.e., 2006).¹⁷ We replicate our empirical strategy for each of these placebo events by assigning firms into treatment and non-treatment groups based on the prior three years of foreign profits. We then examine the differential change in corporate disclosure (*FRQ*) for treated and a set of matched control firms. We perform this test separately for each two placebo events, using the same sampling criteria, covariate matching approach, and definitions of treatment and control groups that we used for the actual AJCA of 2004. If our result that treated firms react to the exogenous relaxation of financial constraints (and their disclosure behavior is not simply the result of an overall differential trend), we would expect to find insignificant estimated ATT coefficients for both of these alternate event windows.

Panel B of Table 5 reports the results of the placebo analysis, as well as the baseline estimates for the true AJCA for reference. The estimated difference in financial reporting

¹⁷Since both independent and explanatory variables are calculated over a two year window, the timing of these placebo events correspond to the first non-overlapping event windows before and after the AJCA.

quality changes across treatment and matched control groups is economically small and statistically insignificant for both placebo events. These findings support our assertion that the tax-induced relaxation of financial constraints affects corporate disclosure following the passage of the AJCA and is not some artifact of our estimation procedure.

3.4. Voluntary disclosure

In this section, we expand our results by estimating the treatment effect of the AJCA on the quantity of corporate disclosure. So far, our analysis has focused on the quality of corporate disclosure associated with mandatory financial statements (i.e., annual filings with the Securities and Exchange Commission). The analysis in this section allows us to investigate whether the relaxation of financial constraints led managers to voluntarily disclose more information than is required by regulators. In particular, we focus on company-issued earnings guidance and show that treated firms provide less earnings guidance following the passage of the AJCA.

The measure of voluntary disclosure we utilize is issuance of earnings forecasts by management. A large literature has used management-issued earnings forecasts to measure voluntary corporate disclosure (e.g., Anantharaman and Zhang, 2012; Leuz and Schrand, 2009), including recent research also examining quasi-experimental settings such as Balakrishnan et al. (2013). The voluntary disclosure literature has demonstrated that this form of communication is perceived as informative by the market, i.e., it is not just cheap talk (e.g., Rogers and Stocken, 2005). For instance, managerial earnings forecasts have been associated with reactions by financial analysts in terms of their own earnings forecast revisions (Lang and Lundholm, 1996), as well as by investors as evidenced by changes in stock prices (Ajinkya and Gift, 1984). Moreover, improvements in transparency at the firm level have been attributed to earnings forecasts issued by management, especially when such forecasts occur frequently and with accuracy (e.g. Kim and Verrecchia, 1994; Skinner, 1994).

Our expectation is that treated firms will provide less voluntary disclosure in response to the exogenous relaxation of financial constraints. As argued previously, the AJCA had the effect of lowering the relative cost of internal funds for firms with unrepatriated foreign earnings. In response, managers of treated firms may be more likely to reduce voluntary disclosure, since they have less of a need to tap external capital markets to raise funds. This expectation is consistent with recent empirical research emphasizing a link between disclosure, information asymmetry, and the cost of equity and debt capital (e.g., Bharath et al., 2008; Lee and Masulis, 2009).

We test this hypothesis using our benchmark identification strategy (see Section 2.2), using earnings guidance as issued by management as a measure of voluntary disclosure as the outcome variable. We extract firm level earnings guidance data from the Company Issued Guidelines of the Thomson Reuters First Call Historical Database. For each firm-year, we count the number of quarterly forecasts and pre-announcements of earnings per share provided by management for the firms common shares. To be precise, in a given calendar year, we record a voluntary disclosure event if management provides earnings guidance before the end of a fiscal period or after the end of a fiscal period but before the actual earnings announcement. If there is no guidance event in First Call in a given firm-year, then we assume the firm is a non-guider in that year and set *GUIDE* equal to zero for that firm-year. *GUIDE* is set equal to one otherwise. We merge *GUIDE* onto the baseline sample of treated and non-treated firms displayed in Panel A of Table 1.

Table 6 shows the implementation of our DID matching estimator with voluntary disclosure as the outcome variable.¹⁸ These represent estimates of the impact of the AJCA on the quantity of corporate disclosure, *GUIDE*. We continue to match treated firms to counterfactuals on the basis of the same set of observable characteristics (*SIZE*, *ROA*, etc.), but

¹⁸We use a linear probability model here, but our estimates are qualitatively similar when we estimate a logit model without firm fixed-effects on the treatment and matched control samples.

we now include the pre-AJCA level of voluntary disclosure in the set of matching variables. Panel A displays the summary statistics for the treated and matched control sample in the pre-AJCA period. Comparing the means and medians of the treated and control samples we see that the mean and median differences between the two samples is minimal, at least in terms of economic magnitudes. Thus, the matching procedure is successful in finding a suitable control group. Notice that roughly half of the treated and control groups provide earnings guidance prior to the passage AJCA.

Panel B shows the main results of the voluntary disclosure estimation. From column 1, we see that in the pre-AJCA period the average level of disclosure between treated and control groups is similar in terms of magnitude and statistically indistinguishable, indicating that the matching procedure continues to work well. Column 2 shows that the voluntary disclosure behavior of the treated and matched control groups differs after the law change. While the average level of *GUIDE* among treated firms decreased by 0.102, the corresponding change among control firms was a decrease of 0.065 (column 3). Both of these estimated differences are statistically significant at the 1% level. These estimates imply that the amount of voluntary corporate disclosure (i.e., the probability of providing earnings guidance) decreased by 0.038 more, on average, for firms with unrepatriated foreign earnings prior to the passage of the AJCA, relative to otherwise similar firms.

Finally, in panel C we replicate the analysis of Section 3.3.2, shifting the date of the AJCA forwards and backwards two years and repeating our experiment on these placebo events. The estimated ATT for the placebo events is both economically small and statistically insignificant, providing support for our identification strategy and assumption that a tax-induced relaxation of financial constraints caused an adjustment in disclosure behavior.

Taking the results of this section together with our previous findings, our evidence is consistent with managers responding to the exogenous tax-induced relaxation of financial constraints by decreasing both the quality and quantity of corporate disclosure. In the next

section, we investigate whether the reverse is true when managers experience an exogenous tightening of financing constraints.

3.5. The 1989 collapse of the junk bond market

In this section, we use the 1989 collapse of the junk bond market as a shock to financial constraints (Almeida et al., 2011, 2013; Lemmon and Roberts, 2010). This collapse in credit supply to junk bond-issuing firms was unanticipated and led to significant tightening up of financial constraints. We use this exogenous contraction of the supply of credit to investigate the impact of tighter financial constraints on corporate disclosure. Using our DID methodology, we find that this shock led firms with a reliance on junk bonds to improve the quality of their financial reporting. Thus, using this alternative and negative shock to financial constraints, we uncover an additional piece of evidence consistent with a positive causal relationship between financing constraints and corporate disclosure.

We begin by briefly describing the events surrounding the junk bond market collapse in the late 1980s and why it presents a plausibly exogenous tightening of financial constraints.¹⁹

The U.S. junk bond (or high-yield bond) market grew rapidly in the late 1970s, accounting for as much as 15% of public corporate bonds outstanding by 1985 (see Table 1.1 of Taggart, 1987). Previously, this segment of the U.S. corporate bond market was small and consisted of investment grade bonds that had been downgraded to speculative grade (“fallen angels”). Soon after 1977, competition among investment banks led some to become willing to underwrite bonds for issuers in this risky market segment. Consequently, U.S. corporations rated below investment grade began to issue a large amounts of bonds. Drexel Burnham Lambert, Inc. (Drexel) emerged as the market leader, in part due to synergies with their established junk bond trading operations.

Beginning in 1989, the combination of three major events led to the collapse of the U.S.

¹⁹For additional institutional details, see Taggart (1987) and Lemmon and Roberts (2010).

junk bond market. First, one of Drexel's key executives, Michael Milken, was indicted for racketeering and securities fraud in March of 1989. Drexel itself filed for bankruptcy in February of 1990, leaving behind a large gap in both underwriting capacity and secondary market liquidity for junk bonds, where Drexel had a significant presence.²⁰ Second, as a consequence of the Savings & Loan crisis, the Financial Institutions Reform, Recovery and Enforcement Act (FIRREA) of 1989 was passed into law. This new legislation impaired the supply-side of speculative-grade credit by restricting thrifts that were regulated by the Federal Deposit Insurance Corporation (FDIC) from purchasing junk bonds. The FDIC also required these thrifts to liquidate their existing portfolio holdings by 1994. Finally, concurrent regulatory action by the National Association of Insurance Commissioners resulted in a large scale reclassification of the securities holdings of insurance companies to below-investment grade ratings status. This led insurance companies—a major purchaser of below-investment grade securities—to withdraw from the junk bond market out of fear of losing business to competitors with higher quality asset holdings (Carey et al., 1993).

We wish to use this contraction in the supply of credit to non-investment grade companies in order to identify the causal impact of financial constraints on corporate disclosure. Importantly, as argued in Lemmon and Roberts (2010), this credit supply shock to junk-rated firms was exogenous with respect to demand-related factors that might also have led to changes in corporate disclosure behavior. In particular, while there was a recession from July 1990 until March 1991, it is not the case that the fall of Drexel and changes in the regulatory oversight of various financial institutions occurred in anticipation of this economic downturn. Moreover, our identification strategy will incorporate a suitable control group that will address potential business cycle changes in reporting behavior and allow us to identify the causal impact of the credit supply contraction on below-investment grade firms' disclosure.

²⁰For more information on Drexel and Milken's roles in the junk bond market, see Benveniste et al. (1993) and Taggart (1987).

To estimate the effect of the junk bond market collapse on the quality of corporate disclosure, we adapt our DID matching approach from Section 2.2. Here, due to data restrictions—voluntary disclosure data from First Call is unavailable before 1995—we focus on financial reporting quality as our main measure of corporate disclosure.²¹ In order to adapt this estimator, we simply need to redefine our sample selection, event window, and treatment assignment.

First, following Lemmon and Roberts (2010), we define our event window from 1986 until 1993, with a balanced number of pre-(1986-1989) and post-event (1990-1993) years. Our sample is selected beginning with the set of all firm-year observations in the annual Compustat database from 1986 until 1993. We require that all firm-years have non-missing data for *FRQ* and the matching variables used throughout our analysis.

Next, firms are assigned to treated and non-treated groups on the basis of issuer-level long-term credit ratings. We use Standard & Poor’s (S&P) own classification of “below-investment grade,” which corresponds to firms rated BB+ or below. These firms comprise our treatment sample. Firms without an S&P rating are classified as non-junk bond issuers and assigned to the non-treated group. This sample selection and treatment classification procedures together yield 190 treated (i.e., junk bond issuing) firms and 890 non-treated firms.²² As before, each treated firm is matched to as many as three unique control firms using a nearest-neighbor propensity score matching scheme, which yields 474 matches to 264 unique control firms. This matched sample is the basis of the empirical analysis in this section.

²¹In addition, due to cash flow data being available in Compustat for only part of this event window, we compute accruals using balance sheet data. We do so using the standard Sloan (1996) measure of current accruals (see also Bergstresser and Philippon, 2006).

²²Following Lemmon and Roberts (2010), firms with an S&P rating greater than or equal to BBB- (“investment grade”) are excluded from the analysis. A simple comparison of investment grade and below-investment grade firms may be misleading, as the investment grade firms are likely to be financially unconstrained and have access to alternative sources of capital. Put differently, investment grade firms are unlikely to constitute a suitable control group and changes in their reporting behavior following the collapse of the junk bond market may reflect unobserved differences between the two groups.

Panel A of Table 7 presents the summary statistics for the treated and matched control sample in the pre-shock period. Comparing the means and medians of the treated and control samples we see that the mean and median differences between the two samples is minimal, at least in terms of economic magnitudes.²³ Thus, we are confident that the matching procedure is successful in finding a suitable control group.

Panel B presents the main estimates of the effect of the junk bond market collapse on financial report in quality. While the average level of *FRQ* among treated firms decreased by 0.013, the corresponding change among control firms was a decrease of 0.003 (column 3). Only the estimated difference for the treated group is statistically significant at the 1% level. These estimates imply that the quality of corporate disclosure increased by more, on average, for firms rated below investment grade following the collapse of the junk bond market, relative to otherwise similar firms.²⁴

For robustness Panel C replicates this analysis using the 1993 to 2000 period and a placebo junk bond market collapse occurring in 1997. This time period is the first non-overlapping window following the timing of the actual 1989 collapse. The estimated ATT for this placebo event is opposite in direction and statistically insignificant, providing support for our identification strategy and assumption that a tightening of financial constraints due to a contraction in the supply of credit caused an adjustment in disclosure behavior for below-investment grade firms.

The evidence presented in Table 7 thus collectively supports a positive causal effect of financial constraints on corporate disclosure. In particular, when financial constraints

²³Note that the difference in the mean level of *FRQ* among treatment and control samples in the pre-shock period does not compromise the internal validity of our estimates in Table 7 because levels are differenced out in the estimation procedure. Internal validity hinges on the parallel trends assumption of our DID estimator. This assumption states that in the absence of treatment, treated and control firms should exhibit similar growth rates in dependent variables. As with every difference-in-differences study, the parallel trends assumption is untestable (Roberts and Whited, 2012). We provide evidence in favor of its validity by showing there are no pre-trends in *FRQ*, as well as conducting a placebo test. For *FRQ*, the difference in medians between treated and control samples is not statistically significant.

²⁴Recall that a decrease in *FRQ* corresponds to an improvement in the quality of corporate disclosure.

exogenously tighten for a subset of firms, we observe this subset improves the quality of their financial reporting. This evidence is consistent with management improving transparency in order to reduce financing costs associated with information asymmetry.

4. Conclusion

In summary, this paper has presented evidence that financial constraints in a firm causes managers' disclosure policy to adjust. Our results indicate that an exogenous, tax-induced relaxation of financial constraints reduces both the quantity and quality corporate disclosure, consistent with a lower cost of internal funds eroding the financing-related benefits associated with greater transparency. We observe stronger effects for those firms that we expect to be financially constrained ex ante, lending credibility to our quasi-experimental research design. In addition, we observe a reversal of these effects when we consider an exogenous tightening of financial constraints for below-investment grade firms following the unexpected collapse of the junk bond market in 1989.

Taken together, these findings lend empirical support to prior research that examines the impact of disclosure on corporate policies (e.g., investment) that explicitly assumes that reporting practices and, consequently, the degree of information asymmetry are endogenously determined. They also highlight the relevance of financial constraints as a determinant of corporate disclosure practices.

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Table 1
Characteristics of the treatment and control samples before the AJCA

This table shows summary statistics for treated, non-treated, and matched control firms for the two years prior to the passage of the American Jobs Creation Act of 2004 (AJCA). Panel A reports summary statistics for the non-treated firms and Panel B for the matched control sample. The treatment sample consists of 489 firms with positive cumulative reported earnings profits from 2001 until 2003. The non-treated sample consists of the remainder of the Compustat universe with valid matching variables (1,149 unique firms). Treated firms are matched to control firms using a nearest-neighbor propensity score match using a 0.005 caliper and matching up to three unique control firms on the basis of the five variables below. This matching scheme yields 1,393 matches involving 589 unique control firms. ***, **, * Denote 1%, 5%, and 10% statistical significance, respectively, based on a two-tailed difference in means test with unequal variances. All variables are defined in Appendix A.

Variable	Treatment sample					Control sample					Diff. in means	<i>t</i> -stat.	
	Mean	Q1	Median	Q3	Std. dev.	Mean	Q1	Median	Q3	Std. dev.			
Panel A: Non-treated firms as control sample													
<i>FRQ</i>	0.08	0.02	0.04	0.10	0.09	0.10	0.02	0.06	0.12	0.12	0.12	-0.02***	-7.06
<i>SIZE</i>	6.67	5.55	6.69	7.83	1.74	5.64	4.08	5.45	6.82	1.95	1.95	1.03***	19.92
<i>MTB</i>	1.51	0.78	1.14	1.72	1.21	1.47	0.76	1.08	1.74	1.21	1.21	0.03	0.99
<i>ROA</i>	0.03	0.00	0.04	0.08	0.10	-0.02	-0.06	0.02	0.07	0.17	0.17	0.05***	13.22
<i>TANG</i>	0.26	0.12	0.23	0.35	0.19	0.30	0.10	0.24	0.45	0.24	0.24	-0.04***	-6.37
Panel B: Matched firms as control sample													
<i>FRQ</i>	0.08	0.02	0.04	0.10	0.09	0.07	0.02	0.04	0.09	0.09	0.09	0.00	0.59
<i>SIZE</i>	6.67	5.55	6.69	7.83	1.74	6.65	5.24	6.51	7.96	1.97	1.97	0.02	0.43
<i>MTB</i>	1.51	0.78	1.14	1.72	1.21	1.46	0.78	1.11	1.78	1.09	1.09	0.05	1.47
<i>ROA</i>	0.03	0.00	0.04	0.08	0.10	0.03	-0.01	0.04	0.09	0.12	0.12	0.00	1.52
<i>TANG</i>	0.26	0.12	0.23	0.35	0.19	0.27	0.10	0.21	0.38	0.22	0.22	-0.00	-0.76

Table 2
The AJCA and corporate disclosure: Difference-in-differences estimator

This table shows results for baseline difference-in-differences propensity score matching estimator for the impact of the American Jobs Creation Act of 2004 (AJCA) on corporate disclosure. The treatment sample consists of 489 firms with positive cumulative reported foreign earnings from 2001 until 2003. Treated firms are matched to control firms using a nearest-neighbor propensity score match using a 0.005 caliper and matching up to three unique control firms, as described in Table 1. The dependent variable, *FRQ*, is our measure of the quality of corporate disclosure. The average level and change in *FRQ* from the two years before the AJCA to the two years after is calculated for treatment firms, control firms, and the difference-in-differences between treatment firms and control firms (ATT). Heteroskedasticity-consistent standard errors are reported in parentheses. ***, **, * Denote 1%, 5%, and 10% statistical significance, respectively, based on a two-tailed *t*-test. All variables are defined in Appendix A.

Dependent variable: <i>FRQ</i>			
	Before AJCA [1]	After AJCA [2]	Difference after – before [3]
Treated firms	0.075*** (0.002)	0.112*** (0.004)	0.037*** (0.004)
Matched control firms	0.074*** (0.002)	0.099*** (0.003)	0.026*** (0.004)
Difference treated – control	0.001 (0.002)	0.013** (0.005)	
DID estimator (ATT)			0.012** (0.006)

Table 3
The AJCA and corporate disclosure: Impact of financial constraints

This table reports estimates of the impact of the American Jobs Creation Act of 2004 (AJCA) on corporate disclosure stratifying the treatment sample using three measures of financial constraints. The treatment sample consists of 489 firms with positive cumulative reported foreign earnings from 2001 until 2003. Treated firms are matched to control firms using a nearest-neighbor propensity score match using a 0.005 caliper and matching up to three unique control firms, as described in Table 1. The dependent variable, *FRQ*, is our measure of the quality of corporate disclosure. The average level and change in *FRQ* from the two years before the AJCA to the two years after is calculated for treatment firms, control firms, and the difference-in-differences between treatment firms and control firms (ATT). Each financial constraint measure partitions the treatment sample in the pre-event window. Firm size partitions the treatment sample according to whether the treated firm had total assets above or below the sample median, dividend payers are treated firms that paid a dividend, and cash flow partitions the sample according to whether the treated firm had cash flow scaled by assets above or below the sample median. Heteroskedasticity-consistent standard errors are reported in parentheses. ***, **, * Denote 1%, 5%, and 10% statistical significance, respectively, based on a two-tailed *t*-test. All variables are defined in Appendix A.

	Firm size		Dividend payer?		Cash flow	
	Small [1]	Large [2]	No [3]	Yes [4]	Low [5]	High [6]
Change for treated firms	0.040*** (0.006)	0.038*** (0.006)	0.039*** (0.006)	0.035*** (0.006)	0.042*** (0.006)	0.035*** (0.006)
Change for matched control firms	0.024*** (0.005)	0.032*** (0.006)	0.023*** (0.005)	0.029*** (0.006)	0.026*** (0.005)	0.027*** (0.006)
DID estimator (ATT)	0.017** (0.008)	0.007 (0.008)	0.016** (0.007)	0.007 (0.008)	0.017** (0.008)	0.008 (0.008)

Table 4
The AJCA and corporate disclosure: Alternative treatment and control assignment

This table reports estimates of the impact of the American Jobs Creation Act of 2004 (AJCA) on corporate disclosure using four alternative definitions of the treatment and control samples. First, candidate treatment firms consist of all firms with positive cumulative reported foreign earnings from 2001 until 2003. In column [1], all such firms are assigned treated status. In column [2], yearly foreign earnings are scaled by assets and averaged from 2001 until 2003 and firms with average foreign earnings above the sample median are assigned treated status. Second, candidate treatment firms consist of all firms with a positive estimated tax savings under the AJCA, as defined in Eqn. (5), from 2001 until 2003. In column [3], all such firms are assigned treated status. In column [4], only firms with estimated tax savings above sample median are assigned treated status. Third, candidate control firms consist of all firms with foreign operations and nonpositive cumulative foreign earnings from 2001 until 2003 (column [5]). Treated firms are matched to control firms using a nearest-neighbor propensity score match using a 0.005 caliper and matching up to three unique control firms, as described in Table 1. The pool of candidate control firms consists of the remainder of the Compustat universe with valid matching variables, except in column [5] where a restricted control sample is implemented. The dependent variable, FRQ , is our measure of the quality of corporate disclosure. The average level and change in FRQ from the two years before the AJCA to the two years after is calculated for treatment firms, control firms, and the difference-in-differences between treatment firms and control firms (ATT). Heteroskedasticity-consistent standard errors are reported in parentheses. ***, **, *, * Denote 1%, 5%, 5%, and 10% statistical significance, respectively, based on a two-tailed t -test. All variables are defined in Appendix A.

Dependent variable: FRQ	Assign treated status if			Assign control status if		
	foreign earnings		tax savings	foreign earnings		foreign earnings
	> 0 [1]	> med. [2]	> 0 [3]	> med. [4]	≤ 0 [5]	≤ 0 [5]
Change for treated firms	0.037*** (0.004)	0.049*** (0.006)	0.038*** (0.004)	0.033*** (0.006)	0.035*** (0.005)	0.035*** (0.005)
Change for control firms	0.026*** (0.004)	0.032*** (0.006)	0.025** (0.004)	0.016*** (0.006)	0.013*** (0.005)	0.013*** (0.005)
DID estimator (ATT)	0.012** (0.006)	0.017* (0.009)	0.012** (0.006)	0.016** (0.008)	0.022*** (0.007)	0.022*** (0.007)
Number of treated firms	489	252	499	234	489	489

Table 5

The AJCA and corporate disclosure: Robustness and falsification analysis

This table conducts a falsification analysis of the baseline estimates. Panel A tests to see if there are common trends in corporate disclosure behavior among treatment and matched control firms in the years prior to the passage of the American Jobs Creation Act of 2004 (AJCA). The annual change in FRQ is reported for the baseline treatment and matched control sample as far as five years prior to the AJCA along with differences in means and medians between these two groups. Statistical significance is suppressed in columns 1 and 2. Panel B considers placebo events in which the timing of the AJCA is shifted forwards two years (AJCA assigned to 2006) and backwards two years (AJCA assigned to 2002). For each placebo event, the treatment sample consists of all firms with positive cumulative reported foreign earnings in the three years prior to the event date. Treated firms are matched to control firms using a nearest-neighbor propensity score match using a 0.005 caliper and matching up to three unique control firms, as described in Table 1. The dependent variable, FRQ , is our measure of the quality of corporate disclosure. In Panel B, the average level and change in FRQ from the two years before the AJCA to the two years after is calculated for treatment firms, control firms, and the difference-in-differences between treatment firms and control firms (ATT) and heteroskedasticity-consistent standard errors are reported in parentheses. ***, **, * Denote 1%, 5%, and 10% statistical significance. All variables are defined in Appendix A.

Panel A: Trends in disclosure prior to the AJCA

Dependent variable: FRQ

	Treatment mean [median]	Control mean [median]	Diff. in means [medians]	p -Value of diff. t -test [Pearson χ^2 test]
Change in FRQ	[1]	[2]	[3]	[4]
2 years prior	-0.007 [-0.005]	-0.002 [-0.006]	-0.005 [0.001]	0.645 [0.822]
3 years prior	-0.113 [-0.007]	-0.089 [-0.015]	-0.025 [0.008]	0.351 [0.132]
4 years prior	0.106 [0.012]	0.038 [0.011]	0.067* [0.001]	0.053 [0.897]
5 years prior	0.009 [-0.001]	0.005 [-0.002]	0.005 [0.001]	0.618 [0.948]

Panel B: DID for placebo eventsDependent variable: *FRQ*

	AJCA year assignment		
	2002 [1]	2004 [2]	2006 [3]
Change for treated firms	-0.043*** (0.004)	0.037*** (0.004)	-0.007 (0.005)
Change for matched control firms	-0.037*** (0.003)	0.026*** (0.004)	-0.014*** (0.004)
DID estimator (ATT)	-0.006 (0.005)	0.012** (0.006)	0.007 (0.007)

Table 6
The AJCA and corporate disclosure: Voluntary disclosure

This table shows results for difference-in-differences propensity score matching estimator for the impact of the American Jobs Creation Act of 2004 (AJCA) on voluntary corporate disclosure. The treatment sample consists of 422 firms with positive cumulative reported foreign earnings from 2001 until 2003. Treated firms are matched to control firms using a nearest-neighbor propensity score match using a 0.005 caliper and matching up to three unique control firms. This matching scheme yields 1,266 matches involving 493 unique control firms. Panel A reports summary statistics for the treated and matched control samples from the pre-AJCA period. Panel B reports the estimates from the matching estimator. Panel C considers placebo events in which the timing of the AJCA is shifted forwards two years (AJCA assigned to 2006) and backwards two years (AJCA assigned to 2002). The dependent variable, *GUIDE*, is our measure of the quantity of corporate disclosure. The average level and change in *GUIDE* from the two years before the AJCA to the two years after is calculated for treatment firms, control firms, and the difference-in-differences between treatment firms and control firms (ATT). Heteroskedasticity-consistent standard errors are reported in parentheses. ***, **, * Denote 1%, 5%, and 10% statistical significance, respectively, based on a two-tailed *t*-test. All variables are defined in Appendix A.

Variable	Treatment sample				Matched control sample				Diff. in			
	Mean	Q1	Median	Q3	Std. dev.	Mean	Q1	Median	Q3	Std. dev.	means	<i>t</i> -stat.
<i>GUIDE</i>	0.458	0	0	1	0.498	0.461	0	0	1	0.499	-0.003	-0.17
<i>SIZE</i>	6.578	5.546	6.589	7.720	1.710	6.482	5.349	6.280	7.691	1.808	0.095*	1.86
<i>MTB</i>	1.566	0.811	1.163	1.802	1.230	1.519	0.815	1.193	1.856	1.077	0.046	1.36
<i>ROA</i>	0.036	0.005	0.045	0.084	0.104	0.029	-0.002	0.036	0.093	0.121	0.007**	2.06
<i>TANG</i>	0.264	0.114	0.220	0.367	0.198	0.251	0.094	0.199	0.350	0.201	0.013**	2.26

Panel B: Difference-in-differences estimator

Dependent variable: *GUIDE*

	Before AJCA [1]	After AJCA [2]	Difference after – before [3]
Treated firms	0.458*** (0.010)	0.356*** (0.010)	-0.102*** (0.014)
Matched control firms	0.461*** (0.010)	0.396*** (0.010)	-0.065*** (0.014)
Difference treated – control	-0.002 (0.013)	-0.040*** (0.013)	
DID estimator (ATT)			-0.038** (0.019)

Panel C: DID for placebo events

Dependent variable: *GUIDE*

	AJCA year assignment		
	2002 [1]	2004 [2]	2006 [3]
Change for treated firms	-0.081*** (0.013)	-0.102*** (0.014)	-0.078*** (0.012)
Change for matched control firms	-0.087*** (0.012)	-0.065*** (0.014)	-0.081*** (0.012)
DID estimator (ATT)	0.004 (0.017)	-0.038** (0.019)	0.005 (0.017)

Table 7
The 1989 junk bond market collapse: Tighter financial constraints

This table examines the 1989 collapse of the junk bond market as an exogenous tightening of financial constraints for below-investment grade firms. The treatment sample consists of 190 firms with a below-investment grade credit rating (BB+ or below) in the annual Compustat database. Non-treated firms include all unrated firms. Each firm in the sample has valid data for 1986-1993 and treated firms do not change status to or from investment grade (BBB- or higher) during the period. Treated firms are matched to control firms using a nearest-neighbor propensity score match using a 0.005 caliper and matching up to three unique control firms. This matching scheme yields 474 matches involving 264 unique control firms. Panel A reports summary statistics for the treated and matched control samples from the pre-shock period. Panel B reports the estimates from the matching estimator. Panel C considers placebo events in which the timing of the experiment is shifted forwards to the first non-overlapping event window. The dependent variable, FRQ , is our measure of the quality of corporate disclosure. The average level and change in FRQ from the three years before to the three years after is calculated for treatment firms, control firms, and the difference-in-differences between treatment firms and control firms (ATT). Heteroskedasticity-consistent standard errors are reported in parentheses. ***, **, *, * Denote 1%, 5%, and 10% statistical significance, respectively, based on a two-tailed t -test. All variables are defined in Appendix A.

Panel A: Summary statistics before the 1989 junk bond market collapse

Variable	Treatment sample		Matched control sample		Diff. in means	t-stat.		
	Mean	Median	Std. dev.	Mean			Median	Std. dev.
FRQ	0.069	0.055	0.043	0.062	0.050	0.037	0.007***	2.86
$SIZE$	5.427	5.298	1.010	5.246	5.042	1.186	0.180*	1.86
MTB	1.042	0.938	0.481	1.069	0.964	0.391	-0.027	-0.56
ROA	0.013	0.022	0.066	0.020	0.031	0.052	-0.007	-1.24
$TANG$	0.368	0.298	0.220	0.366	0.344	0.148	0.002	0.10

Panel B: Difference-in-differences estimator

Dependent variable: *FRQ*

	Before shock	After shock	Difference after – before
	[1]	[2]	[3]
Treated firms	0.069*** (0.002)	0.056*** (0.002)	-0.013*** (0.003)
Matched control firms	0.062*** (0.002)	0.059*** (0.002)	-0.003 (0.002)
Difference treated – control	0.007*** (0.002)	-0.003 (0.002)	
DID estimator (ATT)			-0.010*** (0.003)

Panel C: DID for placebo event

Dependent variable: *FRQ*

	Timing of event	
	Actual: 1989 Before: 1986-89 After: 1990-93 [1]	Placebo: 1997 Before: 1994-97 After: 1998-2000 [2]
Change for treated firms	-0.013*** (0.003)	0.010*** (0.003)
Change for matched control firms	-0.003 (0.002)	0.006** (0.003)
DID estimator (ATT)	-0.010*** (0.003)	0.004 (0.004)

Appendix A: Variable definitions

This appendix presents the definitions for the variables used throughout the paper.

Panel A: Disclosure variables	
Variable	Definition
<i>FRQ</i>	Absolute abnormal accruals computed as the difference between a company's total accruals and its nondiscretionary accruals.
<i>GUIDE</i>	Dummy variable equal to one if a company issues either at least one management earnings estimate or at least one earnings pre-announcement in a year.
Panel B: Treatment assignment and control variables	
Variable	Definition
<i>SIZE</i>	Natural logarithm of total assets
<i>MTB</i>	Market value of assets divided by total assets. Market value of assets is computed as the sum of the market value of common equity (shares outstanding times stock price), preferred stock and book debt minus the book value of deferred taxes. Preferred stock and deferred taxes are set to zero if missing in Compustat
<i>ROA</i>	Return on assets calculated as net income divided by total assets
<i>TANG</i>	Net property, plant and equipment divided by total assets
Panel C: Cross-sectional analysis variables	
Variable	Definition
Dividend payer?	Dummy variable equal to one if a company paid a dividend in the pre-treatment year (2003)
Cash flow	Cash Flows defined as the sum of income before extraordinary items and depreciation and amortization